Stress and Arousal Symptoms in Individuals and Groups - Persian Gulf War Symptoms as a Paradigm

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This report makes a series of recommendations, some general, and some quasi-specific for future patterns of research into the somatic and other consequences of combat stress, deployment stress and other stresses of military life. It is our conclusion that the time has come to move away from the general and only partially meaningful definitions of "stress" to more tightly operational and measurable ones. We also believe that the time has come to initiate research with techniques that will stand the tests of measuring short, mid and long term outcomes replacing present modes of intervention and treatment, which have often been rooted in unproved concepts and psychiatric folklore.

Much of the earlier research into the somatic consequences of stress and indeed into medicine as a whole, was correlational in nature. The value of correlational research and findings remains unchallenged as an indicator and guide to general areas in which causality can be discovered and effective treatment devised. Its utility, however, is limited when it comes to specific preventive or therapeutic interventions. Because correlational research was directed at single levels of the biological hierarchy and unitary causal agents, it has been central to the "medical model" for some generations. The implicit radical reductionism underlying this model has been the source of the greatest success of medicine when dealing with both the elucidation of cause and the treatment of diseases traceable to single pathogens or toxins. Yet even when the single pathogen of a disease is uncovered, the issue of prevention often requires the integration of multiple factors from the molecular to the socio-cultural. Cholera is an apt illustration of the problem of the integration of multiple factors into the treatment and prevention of an illness.

In terms of physical disease, cholera presents a typical example of the discovery of a unitary cause of disease. The prevention of cholera necessitates the need to integrate a variety of factors. Its prevention weaves together sociocultural, behavioral and medical - biological variables ranging from the life cycle of the vibrio to human genetic and physiological differentiation to resistance to the effects of infection. From the point of view of simple correlational epidemiology, potential control of the range of infection began when John Snow defined cholera as a waterborne disease and isolated the water system and the specific pump that was the primary source of the infected cluster in London, in 1854[1]. Snow's work, necessarily *post hoc,* was the first that made possible a limitation of the outbreak following the first wave of illness. Given the knowledge of the time, however, there was no "pre-insult" way of preventing the first wave of an epidemic. It was only after the waterborne source of the disease, the cholera vibrio was discovered, its actions in relation to the physiology of the gut discerned, the development of a preventive vaccine, and the discovery of antibiotics that killed the vibrio along with notice that some individuals had high resistance or immunity to the disease could a coherent program of prevention and treatment be developed. If the organization of behaviors designed to preclude exposure to the pathogen is neglected or breached the population is at risk for ingesting the pathogen. Vaccination provides protection for a majority but not all of the population leaving a substantial segment at risk for developing the disease. Once the disease process has begun antibiotics and rehydration will minimize mortality but days will be lost to the effects of the disease as well as having a significant impact on the medical care system.

As in the case of successful prevention of cholera, the need to integrate multiple factors is particularly relevant to military medicine. The explicit mandate of the U.S. Army Medical Department has always been to "Preserve the Fighting Force". To this original mandate this century appears to have added, "Return the force home with no long term sequelae that might have been avoidable". These mandates carry with them a series of implicit demands. Among the more important are:

1. In so far as possible, prevent the illness from occurring.
2. If exposure to the causes of the illness occurs, intervene to prevent its development and proximate symptoms -if possible.
3. If proximate symptoms have developed, intervene to prevent mid term disability.
4. If prolonged mid-term symptoms and disability have developed, intervene - if possible - to abort the illness process and prevent it from extending into chronicity.

How do we translate these mandates from a cholera model, from a disease we are capable of preventing and are able to treat with a high degree of certainty (but which still remains a threat) to the problems of the psychological and somatic consequences resulting from (what are believed to be) the stresses and trauma of deployment and combat? Somatic symptoms are sets of illness responses still shrouded in ambiguity. They have few verifiable causal chains. There is a high probability that causation for somatic complaints is multi-factorial. Their causation includes the impact of a number of phenomena which are distributed through time and are possibly subject to chaotic influences. Small events may have great impact in the manifestation of somatic symptoms and illness. Preventive measures that have appeared to work in the past, are high levels of cohesion, good supportive leadership, and high levels of training and technical skill. These techniques have significant correlational strength in tests to measure the frequency of stress disorders in the military, but we neither know why they are effective as group phenomena or why they are ineffective for a significant subset of soldiers.

In equal measure we do not understand why some individuals are apparently predisposed to experiencing marked and prolonged, sometimes disabling, symptoms following "stress" exposures while large numbers of others in military service are essentially immune to such sequelae. In a sense this is analogous to the issue of "natural immunity" to cholera. Is this immunity a result of sub-clinical exposure in childhood, a different physiology, i.e. a "stronger" immune system or some other as yet unknown factors? Is resistance to the effects of "stress" the result of "innoculatory" experiences, neurophysiological factors, cultural perceptions, or all of these and other factors operating together?

When we move from issues of prevention to the treatment of stress, the situation is even murkier. Since World War II we have utilized a wide range of treatment interventions, many apposite and some flatly contradictory. The same modest levels of success have been claimed for all forms of treatment. None of the treatment interventions has stood the test of definiteness nor have they demonstrated the kind of outcome probabilities that would lead to the adoption of a "psychological" antibiotic. They have not in many cases even displayed the modest gains of a

cancer chemotherapeutic agent. Some treatments or interventions with seemingly short-term effects demonstrate no long term power as, for example, critical incident debriefing which is presently the most fashionable proximate intervention for exposure to stressful situations.

While the tools we have at present to intervene in stressful situations range from poor to marginally fair we are unable to discard them. They are what we must bring to both the battlefield and the time of peace that follows. We believe, however, that it is now counterproductive for military medical research to continue refining paradigms and techniques that are at best marginal in effectiveness. To this end, following our review of the literature, the national conference, and prolonged discussions and consideration, we offer the following thoughts and general outlines as to the kinds of research that might finally help us come to grips with and measure the problem of stress in a multi-dimensional fashion. We feel that we need the kind of program that might help us understand the multiple strands of causality involved in stress-related illness and thus lead to more effective preventive and treatment concepts and programs.

Since the very concept of "stress" has demonstrated how amenable it is to a myriad of different and often conflicting interpretations let us begin with it.

We face critical problems locating the concept of stress within a biological framework. The term "stress" poses significant linguistic difficulties. Etymologically, the term may derive from the Latin *distringere* meaning to hinder or molest through the Middle English *destresse*, meaning the seizure of the goods of another as reparation for an injury. The term evolved into other meanings including "an oppressed or distressed state . . . anguish of body or mind." One of the primary meanings from Middle English of " *distresse, destresse* applies to physics, being the measurable strain or pressure exerted by external forces on a material object. Stress in this context evolved to its common definition in biological systems: "A physical or chemical, or emotional factor (such as trauma, histamine, or fear) to which an individual fails to make a satisfactory adaptation and which causes physiological tensions which may be a contributory cause of disease.[2] Any essay attempting to lay out our knowledge of all the components of stress and its relationship to biological events such as the generation of somatic symptoms and physical ailments is fraught with difficulty. We must confront central issues both in biology and culture. Belief systems and culturally ordered perceptions contribute to any individual's experience, report and evaluation of stress.

In physics the concept of stress is keyed to measurable phenomena both in terms of input and output. This unfortunately is not the case in human biology. Measurements of the "force" or "valence" or potential capacity of the external events or stimuli to determine their "weight of stressfulness" are subjective, idiosyncratic and idiolectic. Whether the scales used to measure how stressful an event or situation is, are numerical or lexical the responses measured on the scales are metaphoric and show the widest pattern of variation in any given population. Certain events generate wide agreement as to their level of "stressfulness" although they never achieve complete consensus in responses. Other events or situations, although at one point perceived as "extremely stressful", may at another point lose their "toxic" impact or quality through habituation from persistent or prolonged exposure. For example, in many descriptions of

prolonged combat participants noted that their feelings evolved from horror, shock and fear at the initial sight of enemy dead to pleasure. Civil war soldiers would camp on battlefields strewn with corpses and even on occasion used the corpses as head rests after a battle.[3]

Another problem with the measurement of stress is that the weight or valence of a wide range of stressful events and situations may be culturally determined or "loaded". These valences might arise in large part from the unique psychological and experiential history of any given individual, human or animal. Most observers would agree that experiences such as hunger, thirst, cold, oxygen deprivation and certain forms of sleep deprivation constitute "stress-generating" events that could threaten bodily integrity, homeostasis, and survival after wounding. Yet these experiences might be interpreted variously as stressful or non-stressful throughout a given population dependent upon individual values and expectations in the situations involved. They might then be reported differently. We might consider how much more difficult it is, then, to define objective criteria, for self -reporting, to measure the "stressfulness" of symbolic, or anticipated stresses. Perhaps, using an animal model, we could classify the various responses to such stress-generating phenomena.

The simplest model or paradigm might be that of a mouse population containing four segments. One segment or class is conditioned by pairing a flashing light (the symbolic stimulus) with a painful electric shock. The second group is conditioned by the pairing of a buzzer (also a symbolic stimulus) with the shock, the third by pairing both symbolic stimuli (the flashing light and the buzzer) with the shock. The fourth group would receive no conditioning. Obviously, the physiological responses of each segment or class of mouse population to the light or buzzer, the symbol of the paired threat, would depend on its history of stimulus/shock pairing. Three classes of population would react with physiological responses. The unconditioned segment would ignore the threat, the others would respond to the learned interpretations of buzzer, light or both. This stimulus-response model is, of course, elementary. The biological systems involved in these responses are part of the genetic constitution, the genotype, of the mouse, developed throughout the evolution of the species.

But living organisms are also defined as open systems. As Mayr has put it: "living organisms represent a remarkable form of dualism. and it arises from the fact that organisms possess both a genotype and a phenotype." Through the use of their senses, living organisms take in and process a wide range of external stimuli, events and experiences. Responding to these stimuli, organisms experience highly patterned responses within the body, and outwardly the organisms demonstrate various behaviors in their respective environments. Obviously, among the stimuli are those that (particularly in the last sixty years or so) we have denominated "stressful" or "stressors", such as the shock to the mouse's feet. Just as the electric shock experienced by members of the mouse population is responded to differently by the highly organized biological systems that comprise each individual mouse, within each class or segment of the mouse population there will also be variation, ranging from the subtle to the extreme. Some animals may respond with massive and prolonged behavioral collapse others with fairly low level and short-term responses.

Evolutionary biology teaches us that there is a great deal of variation between members of any species. Humans and animals even when brought up in the same confined space or

family, seldom, if ever, share identical experiences and learning histories. Only cloned animals and identical twins share genotypes and the different experiences of twins can lead to marked differences in their response to the same stimuli or phenomena. Evolution allows for wide individual variation within the species genome. In this sense both the stimulus (the stressor) and the pattern of response - both within the individual and without (its externally directed behavior) - represent the result of complex interactions across the biological hierarchy. In humans this means operating in a both complex and multiplex domain, where many signals must be perceived and processed and then must be assessed. Once assessed, the signals must be given a meaning, and assigned a valence and a consequence. A signal, now decrypted, will be transduced into other signals with probable outcomes that bear on the individual's survival and maintenance of homeostasis. This decryption, done in the brain, then leads to a series of signals sent by the brain through the various physiological/neurophysiological systems of the body. They cascade downward to the molecular level. The molecular, physiological/neurophysiological, messages may then be reinterpreted by the brain, and expressed externally and behaviorally, through language, metaphor and other behavioral responses such as fear, anxiety, elation, etc. The messages may also be expressed, as in the case we are concerned with here, in the generation of somatic symptoms.

The mouse paradigm is based on *a priori* assumptions about how genotypic, phenotypic, historical and environmental differences enter into the issue of stressor and outcome. This simple and reductionist approach yields valuable results at the well-demarcated levels of various sub-systems of the mouse or other organisms but it has minimal value for understanding the behavior of more complex systems and of more complex organisms. For example, the relationship between adrenal secretion and heart rate is fairly predictable for almost all humans, but both the factors that lead to the increase in adrenal secretions and the consequences for the individual of the brain's interpretation of the increase in heart rate will be diverse. In an equally simple fashion the interpretation and consequences of hunger and its status as a stressor will be markedly different for the religious devotee of fasting, who is fasting for three days as a sacred obligation, than it will be for a mountaineer or cave explorer stranded for three days without food. While the proximate body responses to the lack of food for the three days will likely be the same for the explorer and religious devotee, the responses of other bodily physiological systems along with allied neuro-transmitters, now evoked and activated, will undoubtedly be very different. Despite the complexities of multi-factorial stimulus and response, illustrated by the example of hunger discussed above, we can make general predictions of the effects of stressors and responses. This can be done by developing a set of stressors, and then laying out the specific relationships when stressors of different valences are transduced by the brain producing the neurophysiological alterations that may lead to illness and somatic symptoms.

In trying to define a model of stressor and its evoked responses both physical and behavioral, we have to remember any individual human being is a particularly complex, multi-layered system of systems. We must underline again that it is doubtful that any reductionist, one-directional and essentially mechanical model of causality keyed to a singular precipitant will either explain or comprehend all the phenomena or outcomes with which we are concerned. In human beings information moves multidirectionally and involves extensive networks of both

feedback and feed-forward loops. Causation, when established, particularly for stress-implicated phenomena, is usually multi-factorial. Epidemiological and correlational population studies indicate that some factors contribute significant and often extensive amounts of variance in certain pathological outcomes. But we cannot, *a priori*, predict the outcome for any single individual unless we have reasonably accurate knowledge of that individual as a biological entity including his/her environmental history. This knowledge would enable us to characterize the particular individual psychologically, socially, and culturally.

Two examples come to mind. In the classic Framingham Study, focusing on cardiac risk, the study revealed heightened risk for cardiac illness arising from factors such as stress, defined as generated by lack of control in the workplace, and Type A behavior. The risks for any given individual, however, were dramatically mediated by other factors such as genetic endowment, socio-economic status, span of control over life situation, smoking, etc. Even in the more (supposedly) clear-cut situation of H. Pylori, the causative agent of most peptic ulcers, the relationship between cause and illness is less than crystal clear. The bacterium is present in almost 80% of the American adult population but causes ulcers in only a modest percentage of those who harbor the bacterium. There is also a moderately sized sub-population of individuals with peptic ulcers in whom the presence of H. Pylori cannot be determined. In the former case the microbe is a necessary but not sufficient cause of the subsequent illness. In the latter, the bacterium's absence demonstrates that other factors, exclusive of the "pathogen" are responsible for the outcome of the individual having ulcers. The above examples demonstrate that there are only a very limited number of illnesses or outcomes with somatic symptoms in which the experience of a stressor, particularly a psychosocial one, is sufficient to explain the response as experienced by any one individual. In the examples above neither "stress" nor the pathogen H. Pylori alone were sufficient to explain the disease outcomes. Both are requisite. In a majority of the cases the stressor is best defined as a necessary (but not sufficient) component in a complex system.

Acute and Chronic Stresses

One of the definitional problems with the concept of stress lies in the stressor. Stressors may be brief and time limited ranging from passing hassles to overwhelming and traumatic events. Stressors may be continual, that is, events which kindle stress responses repeated at intervals of varying lengths. In addition, the effects of a stressor may be continuous or nearly so, as, for example, in a prolonged period of grief after the death of a spouse. Equally problematic is the fact that the stress response may be evoked during the experience of a stressor, after that experience, or in anticipation of that particular experience or series of events. It may be evoked secondarily through the apprehension of a symbolic representation of the stressor or event. Such symbolic representations may in themselves become a source of the continuing evocation and exacerbation of a state of both bodily and psychological distress. This enormous set of possibilities has reinforced the confusion and lack of clarity inherent in the concept of stress. Oversimplifying the issues involved we might consider the following three patterns as paradigmatic of stress – stressor response relationships.

1.The first pattern would be that of the initial and proximate response to a major threat, or stressor, real or symbolic, which evokes a significant psycho-physiological response, the equivalent of Selye's 'General Adaptation Syndrome' or GAS.[4] The response to such an event may be transient and the organism returns to its pre-existing homeostatic state immediately upon the passing of the (stimulus) situation. For some, however, such events may precipitate periods of 'distress', i.e. continuing dysfunctional, psychological and psycho-physiological behavioral alterations of varying durations lasting days weeks or months.

2. The second example would be a significant psychological and physiological response to exposure of a sequence of stressors over a delimited period of time leading to behavioral breakdown and the exhibition of a wide variety of symptoms. In military terms, the sequence of stressors or precipitating stimuli appears to be subject to a time-intensity trade-off. The concept of 'combat fatigue' derived from World War II and Korea illustrates this trade-off. The critical driver affecting the numbers in a population who would experience a breakdown was the number of pulses of combat (the experience of 'major stressors') during a given period of time. In the Normandy campaign, Swank and Marchand estimated 30 days to the generation of such casualties.[5] . In the Italian campaign, where there were fewer daily pulses of combat, the length of time until psychiatric casualties were generated was longer. (Population diversity, of course, means that numbers of individuals became casualties during the intervening period.) For the majority of such cases the combination of behavioral symptoms (including severe performance decrement) and physical symptoms was transient and readily treatable in 1 to 3 days with a combination of rest, hot food, hydration, encouragement and simple forms of psychotherapy. For these people, the term fatigue, originally adopted to minimize psychiatric or moral stigmatization of these casualties may not have been a misnomer. A sizeable minority, however, were not readily restored through such treatment. Their dysfunctional status and state of distress were prolonged, as were their somatic symptoms including those classified as conversion disorders. A second population tended to become highly symptomatic after much prolonged combat exposure and were referred to with the informal diagnostic classification of 'old sergeants syndrome'. This group, whose symptoms were most consonant with today's diagnostic criteria of PTSD, was difficult to treat. Few were restored to duty and many continued to exhibit psychological and/or physical symptoms for long periods of time.

3. The third pattern is one that might be called 'prolonged, chronic stress/distress'. The stressors involved are usually neither acute nor traumatic but continuing and cumulatively 'erosive'. Such situations appear to generate long-term patterns of psycho-physiological distress in certain sub-populations. One important aspect of such situations lies in the ways that humans may attribute health or life threatening consequences to events that in and of themselves carried no biological or toxic consequences but were reacted to as stressors. [6]

A number of other stress-response-evoking paradigms might be considered, but all, in order to have pathological or symptomatic consequences, would appear to share one major characteristic - the requirement that the psycho-physiological and psychological consequences of the response-evoking event continue beyond the immediate temporal 'frame' of the event itself. In terms that we have chosen to use here the responses move from the short-term phenomena of the GAS into a longer term, continuing 'distressful' pattern.

Review of the "stress" literature demonstrates the proclivity within both the scientific community and the public at large to focus more attention on acute and dramatic stressors in ethnographic, ethological and experimental work. We recommend that the spectrum of research observation and analysis should be broadly expanded. Past research with its reductionist biases and the ever present lure of the dramatic has most often focused on the singular effects of electric shocks, immersions in ice water, or the life-threatening horrors of the battlefield or major natural or man-made disasters. The ease of such focuses arises from the fact that these short-term major stressors so aptly fulfill the classical requirements of the A-B-A experimental design and have served well in animal experimentation and research. However, the most common design in research on precipitating events tends to create a model and mode of thinking that focuses on the 'catastrophic'. The catastrophic mode occurs to a significant degree in nature but also functions as an explanation of the consequences of the event. The event is viewed both as the trigger and the 'cause' of the process that leads to systemic breakdown or degradation of psycho-physiological homeostasis. This 'catastrophic' focus distracts attention from the more complex processes of erosion of system integrity that may follow such a catastrophic event or come on the heels of the myriad hassles and minor stressors which in themselves may be a product of the event, but which may be masked or unattended to by the overwhelming metaphoric and emotional power of the event itself. Several examples from the domain of "life events" studies, as developed by Holmes and Rahe and others demonstrate the way in which the event as metaphor may be reified.[7] The birth of a child into a family can be considered a stressful life event and retrospectively may account for a significant amount of the variance in analysis of stated "stress levels" or numbers and intensity of "stress symptoms" experienced by a given respondent. The event, however, is primarily the descriptor of a wide range of possible stress-evoking and stress-mediating stimuli, such as sleep deprivation, fear and apprehensiveness, cuddling, etc. The birth of the child is causal only in the sense that it creates the possibility for a set of life conditions (stimuli) that may evoke 'stress responses' in the brain and body. The value of any one of these stimuli to evoke such responses is, again, dependent upon such factors as parental history, the social systems pertaining to infant care and cultural beliefs about post-partum events and infancy. Thus the first baby born to new parents is, for many, a source of constant anxiety. There are multiple nighttime and early morning awakenings to "see if everything is alright" and to determine if the infant is "still breathing". Such behavior is seldom evinced with subsequent children. A colicky child may be source of significant stress for many parents, but far less so, if at all, for the well-to-do, where a nanny absorbs the burden. In northern Thailand the first thirty days after birth are a time of great anxiety for the mother because of beliefs that a special wind (*lom pit duen*) may rise within her and lead to extreme illness or death. This fear is not applicable to Americans.

The examples from childbirth are cited for one critical reason. They demonstrate the shortcomings of correlational studies, (which characterize the bulk of the research done in the military). Simply taking Holmes's and Rahe's original description of childbirth as a stressor does tell us something about the level of risk for a population. It tells us nothing about the actual effects or dynamics of such risk. We do not know what the processes are that lead some women to increased risk for later illness because of the stress experience of childbirth while others are

"immune". Correlational studies are of great value indicating where risks abide and in demarcating segments of populations that may exhibit higher probabilities of risk than others, but they tell us little about the etiology of the 'stress response' and the experiencing of distress, other than where to look to for the phenomena. Correlational studies tell us about arenas or risk for populations but reveal little about the ways in which such risk translates into psycho-physiological disruption of the organism's homeostasis. Since the majority of the life stresses to which humans, and especially military personnel, are subjected to cannot be prevented, these studies are of little help in developing preventive techniques.

The knowledge that the number of pulses of battles increases the risk for the numbers of those in a given unit who may experience "combat fatigue" provides us with no preventive tools since the enemy's capacities and abilities control the intensity of combat. We may know more about risk but we will not have advanced our knowledge of preventive techniques or how and why some soldier suffers breakdown and others do not. We may know that following intense combat we might need more psychiatric assets but we do not have advance knowledge of what they should do.

SOMATIC SYMPTOMS AND SOMATIZATION

It is a common occurrence for (primary-care) physicians to attend to patients who present multiple physical symptoms which have no determined pathological basis. Unlike classical somatization disorder, (as defined diagnostically by the Perley - Guze criteria for Briquet's syndrome and the criteria for the D.S.M. IV) which is limited to a narrow spectrum of patients, the presentation by primary care patients of somatic symptoms of undetermined etiology for which no medical basis can be found is commonplace in medical practice throughout the world. A recent WHO study carried out across a number of national and culturally different sites demonstrated the widespread presence of clusters of somatic symptoms related to psychological distress.[8] One of the more interesting aspects of this study was the attempt to determine whether there was a difference in the somatic symptoms related to depression as opposed to anxiety. This cluster of symptoms might bear a relationship to stress. The symptoms might be generated through prolonged psycho-physiological arousal as a result of exposure to stressors. The stressors here might relate more directly to the dysregulatory functions of the prolonged arousal than to the specific psychological matrix from which they might have derived (depression, anxiety etc.)

An important complement to studies depicting the wide occurrence of somatic symptoms related to psychological distress in the primary-care setting is the study of Kirmayer, et. al, which demonstrates the masking effect of somatic symptoms in respect to psychological distress.[9] In these studies carried out in Montreal, the general, and powerful finding was that the more somatic the presenting complaints the less likely the physician was to recognize the underlying depression or anxiety disorder that could readily be diagnosed in the patient.

Scientists, psychiatrists and others have defined somatization as a metaphoric presentation of distress created by emotional and psychosocial problems. According to Katon, et. al. patients "articulate their distress primarily through physical symptomatology [when they]

either do not have discernible organic pathology or [they] amplify their verifiable physiologic changes".[10] Kirmayer and Robbins building on this work have distinguished three salient forms of somatization in primary care settings. 1) "High levels of medically unexplained symptoms reported occurring in multiple physiological systems, an extreme form of which is DSM-III-R somatization disorder." 2)"Levels of somatic preoccupation or illness worry beyond what is expected for demonstrable physical disease." 3) "The predominantly or exclusively somatic clinical presentation of a psychiatric disorder, most commonly depression and anxiety."[11]

Using the DIS interview schedule Kirmayer et. al.. studied 685 outpatients at a hospital in Montreal and attempted to develop operational definitions of somatization patterns in the population.[12] They chose Escobar's more liberal criteria for a diagnosis of "somatization" as opposed to the DSM criteria for "Somatization Disorder. [13] The DSM requires 14 symptoms for women and 12 for men, while Escobar's construct requires four for men and six for women. The criteria of at least four or six medically unexplained symptoms were used by Kirmayer and his colleagues' to define the first pattern which they called "Functional Somatization". Their second category was defined as "Hypochondriacal Somatization" and was based upon the illness worry measure of the DIS combined with the lack of a disease diagnosis or the lack of severity in the disease diagnosis. The third category "Presenting Somatization" involved those who presented somatic complaints that the clinician could attribute to a concurrent psychiatric disorder.

The results of this study are of particular interest since this population presenting physical symptoms to a hospital clinic is somewhat analogous to populations entering military medical systems and also presenting physical symptoms following events like the Gulf War or Vietnam Conflict. Using Escobar's liberal criteria and those for "Hypochondriacal and "Presenting Somatization" a total of 26.3% of the population in the Montreal study had at least one form of somatization. (180 out of 685). In addition, 114 subjects (16.6%) met the 4/6 functional somatization criteria, 53 (7.7%) met the criteria for hypochondriasis and 75 (10/9%) fit the DSM III criteria for major depression and/or anxiety. The Venn diagram showing the overlaps between the three groups still shows significant numbers who fall into in each discrete pattern. Thus 81 of the 114 persons who met the 4/6 criteria called "functional somatization", were "pure" somatizers. While a significant majority of the functional and presenting somatizers were female 7.5% of the total somatizing population was male.[14] Researchers were unable to elicit causation for a majority of the cases. The apparent lack of causation might represent socio-culturally-defined patterns of denial or it might indicate a certain amount of decoupling between physiological and psychological expression of certain phenomena.

Another study, the NIMH Catchment Area Study, looked at a household sample rather than a care-seeking sample. The study found that 4.26% of its population presented three or more symptoms of somatization. The study also found that when symptoms increased there was a high correlation between psychiatric disorders and higher numbers of somatic symptoms.[15] It must be emphasized, however, that this was not a care-seeking population, which would account for its major differences with the other cited studies. Nor was it a study with a post-combat or "post-event" population (such as that of Three Mile Island) which had high exposure to external stressors. The differences in these rates of the NIMH group and those of the self-selected population, concerned enough about the meanings of their symptoms to enter a primary care

systems and the general population is underlined the above cited review article in 1984 by Katon, Ries and Kleinman. Their data indicated much higher levels of somatization. The authors noted that "studies from health maintenance organizations like Kaiser-Permanente have revealed that as many as 60% of primary care patients present with recurrent somatic symptoms that are an expression of psychosocial distress." Furthermore they go on to note that, "Analyses of the content of general medical practice have shown that 68% to 92% of the patients do not have a serious physical disorder. Only 41% of the identified problems of patients have a clear somatic diagnosis and the most common single diagnosis in general practice is non-sickness. Ten to 60% of the patients with each of the five most common medical complaints do not have any identifiable disease responsible for their symptoms."[16]

It is important, in this context, to point out as Robbins and Kirmayer did, that "Indeed, in many cases symptoms never receive a diagnosis but come to constitute health problems in themselves."[17] This is a reasonable probability, as Kellner underlined, that physiologic activity is apparently responsible for many of these symptoms and that they are exacerbated by emotion.[18]

CAN WE BUILD APPROACHES TO THE PROBLEM?

Concepts generated out of many of these studies on somatization were discussed, presented at our conference. One of the conclusions that participants in that conference on the **Somatic Consequences of Stress** (June, 1998) came to was that the knowledge base required for an explicit research program to isolate the unique pathways leading to somatic symptom outcome as a result of exposure to external stressors did not yet exist. There was, however, agreement that the problem of defining stress/distress and its relationship to the generation of somatic symptoms should continue to be approached in terms of the wider phenomena involved in the transformation of external events (stressors) into internal responses that lead to symptomatic expression. These may in turn lead to the generation of more symptoms or the amplification of existing ones. These responses might be the product of psychological, physical, or combined symptoms of varying durations. They provide the first data that then can be classified and analyzed in terms of different classes of symptomatic outcomes. In the body of this report we shall attempt to lay out, albeit crudely, an approach to research areas at various levels of interlocking systems or domains that appear to characterize the combination of psychological, psychosocial and neurophysiological processes involved.

BUILDING AN INTEGRATED MODEL AS A SCAFFOLDING FOR FUTURE RESEARCH

1. The Requirement for an Operational Definition of Stress

As we indicated in the introduction, the concept of "Stress" in amorphous and ambiguous. Its primary utility, scientifically, has been conceptual. The concept has been a powerful analytic tool, but less useful in actual analyses because the concept is non-standardized, and thus serves as a weak variable. The very amorphousness and lack of precision in the concept of stress have led many in the general public as well as in medicine to view it as another of the

vague and non-falsifiable variables more appropriate to New-Age and Alternative Medicine rather than rigorous usage in allopathic medicine. While the problem is complex and difficult it may not be an insuperable one.

We do not intend to do a wide historical review of the evolution of the "stress concept". The roles played by Walter Cannon and Hans Selye in its establishment as well known[19]. Also well known is the role played by the U.S. Military psychiatrists during and after World War II, particularly, the work of Harold Wolff in establishing the stress concept as part of medical thinking about illness and somatic and behavioral symptom generation.[20] The concept, growing out of Selye's work on what he defined as the General Adaptational Syndrome [GAS] was keyed to Selye's definition of stress as the "non-specific response to any demand".[21] Selye's explication of the concept was that "If we abstract from . . . [the body's specific homeostatic mechanisms' reactions]. . . there remains a common residual response that is non-specific as regards its cause and can be elicited with such diverse agents as cold, heat, x-rays, adrenalin, insulin, tubercle bacilli or muscular exercise. This is so despite the essentially different nature of the evocative agents themselves and despite the coexistence of highly specific adaptive reactions to any of these agents."[22] The very elasticity of Selye's definition as well as its implicit confounding of stimulus and response was strongly challenged in the seventies by Hinkle, Mason and others.

In 1973 Hinkle severely criticized the concept of stress as a "bodily state". Basing his conceptions on the work of Shannon, Weaver and other information theorists Hinkle pushed strongly for the adoption of an information-based model that would enable analyses of "stress' to move from the non-specific concept of stress to a quantifiable one.[23] Hinkle noted that "In man and in higher animals, reactions to the environment which are mediated by the sense organs and the central nervous systems have the capacity to influence any process within the organism that can be influenced by the gross motor behavior of the organism itself, or by the alteration of any function of either the organism or of its component parts, which can be influenced by the skeletal or autonomic nervous system, or by the glands of internal secretion, acting alone or together."[24]

Hinkle went on to make several major points which are critical to present thinking about the stress concept. Hinkle noted that major "life changes" are not necessarily associated with disease. During World War II the expected epidemics of disease did not appear in the population of London during the rigors of the "Blitz" in the 1940s nor did epidemics break out amongst those evacuated to safer areas. Interestingly, these findings were confirmed by in a wide scale "Strategic Bombing Survey" carried out by the U.S. after World War II. The survey demonstrated this was substantively true for all the major belligerents in the war. Unfortunately, it was often ignored in subsequent psychosocial stress research. Hinkle went on to observe that, "In view of the fact that people react to their "life situations" or social conditions in terms of the meaning of these situations to them. it is difficult to accept the hypothesis that certain kinds of situations or relationships are inherently stressful and certain others are not..." Finally, Hinkle observed:

That the relation of people to their society and to the people around them can influence the incidence, the prevalence, the course and the mortality of diseases seems clear enough. The questions at issue are questions of when they do so, under what

circumstances, by what mechanism and to what extent. A question likely to remain moot is the question of whether or not a social condition or an interpersonal relationship can ever be a "sole and sufficient cause" of disease. The complexity of this question is such that a clear-cut answer undoubtedly will not be forthcoming at any early date. However, the bulk of experience to date suggests that this answer, when it is finally forthcoming, will be "no".[25]

Hinkle's emphases on the meaning of the event as a contributory "effector" of risk for disease should be viewed in tandem with Mason's 1975 critique of Selye's concept of the non-specificity of stress. Mason's critique focused upon the consequences of stress becoming specific and measurable. Mason pointed out that:

> " . . . Of all the known responses of higher organisms, *emotional arousal is certainly one of the most ubiquitous or relatively "nonspecific" reactions common to a great diversity of situations.* The fact that such emotional arousal is, in turn, known to be characteristically associated with many hormonal changes may well provide the most plausible explanation at present for the high frequency of adrenal cortical responses in laboratory situations involving "noxious" stimuli or stressors. In other words, *this distinction fundamentally changes the view from that of a hormonal response being elicited by a great diversity of stimuli to that of a hormonal response being elicited by a single stimulus or stimulus class, common to a great diversity of situations, namely the ubiquitous factors which elicit emotional arousal."*[26]

The evolving lesson that specific hormonal and other physiological changes took place in response to external events was well understood both implicitly and explicitly by experimental scientists. It underlined the fact that that gross correlational studies and *post hoc* studies do not advance our scientific understanding of the dynamics and mechanics of the state called stress or being stressed. Thus it is critical for research and experimentation to focus on those known measurable responses of the body, (as developed through this century out of the work of Cannon and Wolff[27]) as mediated by the brain's interpretation of stimuli and events.

In 1987, Fleming and Baum described a multi-dimensional approach to the "measurement of stress" or rather the stress response, collecting "simultaneous measures of psychological, behavioral, physiological and biochemical states". Their research model used self-report, behavioral, physiological and biochemical measurements[28]. We can use an updated model of the version they proposed. We can utilize it to create a set of reasonable metrics for looking at the external affecting event (the stressor) and the contingent stress responses experienced by members of human populations. We may thus develop more valid risk criteria for the populations at risk for somatic symptoms and future illnesses.

Research Prospects

As we have noted, there is wide variation in the reactive predispositions of human populations. Given this wide variation, it will, for the foreseeable future, probably be impossible to design a 'one-size-fits-all' measuring tool that can predict or indicate a dysregulating [or deleterious 'stress response' which carries with it a high risk of illness and/or severe performance deficit. It may be possible, however, to develop indicators for subsets of populations, that can reasonably predict probabilities of risk for the subsets, when the populations are subjected to various categories and valences of events that are stress-response- evoking stimuli.
Before going on to outline possible research programs to develop the above-discussed indicators, we have to confront certain unknowns, that is areas in which we still lack cogent baseline data. Some of the unknown factors deal with the organism at rest; i.e. in a state not subjected to significant stressor stimuli. There is no firm data as to whether the characteristics of the organism at rest provide physiological, or in many cases psychological, markers of probable future risk. Other unknowns derive from the number of studies and the sizes of populations studied. Few studies have effectively combined the data derived from psychosocial and biological systems as ongoing interactive processes in humans and more pertinently most major studies have been post-event. Most of the studies that have attempted to look at change from a resting state through a response to an intervening stressor have looked at one time event stressors such as Meyerhoff and his colleagues' examination of soldiers appearing before "soldier of the month" or promotion boards. These studies have high symbolic valence to some of the participants but are studies of events of short duration which are not massively threatening. While these studies are of real value and have added considerably to our knowledge of the effects of stressful events, their numbers are small and they cannot provide the basis for making viable probability estimates about populations exposed to chronic distressing events or acute and substantively threatening ones.[29] Evidence gathered in a military context (such as the panel study of troops deployed to the Persian Gulf in Desert Shield/Desert Storm) demonstrated that those soldiers at greatest risk for high levels of post-combat symptoms of psychological distress were those who had high symptom levels and high stress-provoking perceptions of the environment before combat.[30] A significant minority, however, displayed low distress symptoms during the chronically stressful pre-combat deployment, but were highly distressed by their combat experiences. Many members of both groups equivalently reported being highly distressed by various events following their return home. No physiological measures had been taken and no pre-deployment psychological or psychosocial measures were available.

Our working hypothesis suggests that the 'stress response' and the level of probability that such a response or sequence of responses will evolve into a long-term pattern of (physiological) distress and system dysregulation results from the interaction of the individual's physiological and neurophysiological systems with his or her psychological history and social and cultural perceptions and expectations. Any study to test this hypothesis must be a prospective one, in which individuals are tracked and serve as their own controls. Technologically, we are not capable of mounting such a study at present. We could however, design a study using available indicators that would produce a preliminary set of measurements.

These measurements might yield a reasonable number of valid predictions of health outcomes. Rather than generating population qua population probabilities of risk, as did the Alameda County Study, we might be able to define a more finely grained array of population subsets, particularly within the military, for studies of preventive interventions[31]. It must be remembered that there are no wide- scale prospective epidemiological studies that link psychological and psychosocial states with physiological and neurophysiological responses. Du Puys' studies, for example, using the General Well Being Scale on national samples in various iterations of the National Ambulatory Health Survey demonstrated a fairly strong statistical link between psychological distress and risk for physical illness in the ensuing 18 months.[32] We do not know whether this predictive effect would be significantly enhanced if it were coupled with data indicative of the physiological parameters of the 'stress response' and the levels of physiological 'dysregulation' in the individuals undergoing examination. It seems likely, however, that combining data from reports of psychological distress, illness outcomes, and tests that measured physiological changes occurring with stress responses and system dysregulation would provide greater effectiveness in predicting which soldiers would be at greatest risk.

In general and fairly gross terms such a study might look like the following:

The target population would be two military units of battalion size, the majority of whose members could be reasonably expected to remain together for at least two years. The units to be chosen should be two that plan to have several highly demanding training and deployment events scheduled during the research period. The initial step should be surveying the units to determine whether members of the units agree on the levels of perceived demand on the units and their members as well as on the stressfulness of the various operations regularly carried out by the units through their annual cycle. Data collection should then be carried out during the period most widely agreed upon as being the least stressful.

1. For several days prior to initial collection of psycho-social and physiological data the entire group, if possible, or a reasonable sample, should be equipped with actigraphs to determine the actual activity and rest levels that characterize the period.

2. If feasible, the subjects equipped with actigraphs should also be equipped with a non-invasive heart monitors preferably based on a pulse sensor and combined with the actigraphs. The heart rate monitor and actigraph should operate on a common synchronized time-base in order for there to be as perfect a correlation as possible between the measurements of activity and those of heart rate and cardiac acceleration. If this type of monitor is not yet in production, it should be developed. It lies well within the capacities of present chip and sensor technology.

If the theoretical considerations for such a combination of heart rate measurements and actigraphy are as valid as they appear to be, based on presently available empirical, clinical, and anecdotal data this combination might ultimately serve as the primary form of measurement. We could use this combination to measure physiological changes, transitions to stressful response and probable risk for longer-term acquisition of distressful symptoms, patterns of the development of illnesses in field studies of troops. This measurement's potential importance is underlined by two recent studies carried out by Shalev and his colleagues. The first study demonstrated that "Physiological activation during stressful events may play a central role in the pathogenesis of PTSD". The study involved a four-month follow-up of patients and

demonstrated that elevated heart rate in the emergency room and one week thereafter was a significant predictor of the development of PTSD. The elevation in heart rate was independent of the "seriousness" of the traumatic event. While the heart rate finding is clear it is, to a degree, clouded by the lack of baseline data existing prior to the traumatic event. As the authors pointed out, "Our study does not exclude the possible effect or a prior trait (e.g. hyper-responsiveness) on both heart rate response and the development of PTSD[33]. This is a lack that a well-designed cohort study would fulfill. The need for baseline data is underlined by another study of Shalev's that demonstrated that while heart rate predicts the development of PTSD it does not predict the development of posttraumatic depression. This finding may posit a greater specificity in "precursor" neurobiological events that might ultimately be of great analytic importance[34].

3. At a set time, chosen by the neurophysiologists involved in the study, bodily fluids containing target substances of interest should be collected. Such collections should be non-invasive whenever possible. Ideally, immunological markers would also be collected. Saliva to assay for cortisol and amylase might be the most reasonable, if not necessarily. the best indicators to utilize.

4. After fluid collection, each individual would be given a psychosocial battery of tests. The psychosocial battery of tests would be given in place of questionnaires whose anticipatory effects might lead to increased psychological stress. These tests might consist of

a. A present physical status and symptom inventory.

b. An instrument measuring psychological and psycho-physical distress such as the S.C.L. - 90 or its subset the BSI.

c. An inventory of perceived inter-current life stressors, e.g., marital problems, debts, familial illnesses etc., with a subjective evaluation of their levels.

d. Assessment of the vertical and horizontal cohesiveness of the unit and its sub-units.

e. A life satisfaction scale.

This data should be collected at regular intervals and at target periods of high demand during deployment and training.

5. Ideally, an ethnographer/participant observer should be attached to each unit for the duration of the study in order to record as objectively as possible, relationships, patterns of interactions, the 'round' of events, soldiers' perceptions of events, and their cultural patterns of attribution of "stressfulness" to events and situations.

These studies should be paralleled with a series of small group laboratory studies organized around a series of "stressful" tasks. An effective model might be one based on the laboratory of physiological sociology set up at Stanford by P. and J. Barchas, or other small group studies .[35] The 'matrix' of human volunteers might consist of the following: individuals with high psychological symptom levels and high physiological reactivity and prolonged arousal; individuals with low psychological symptom levels and low physiological reactivity and arousal periods; individuals with high psychological symptoms levels and low physiological reactivity and arousal, and individuals with low psychological symptoms levels and high physiological reactivity and prolonged arousal.

The object of these studies would be to establish clearly the chain of factors involved in the psychosocial and physiological chain that conduces to distress and other sequelae. If we could use additional, more finely detailed analyses and other technologies (such at PET scans, if appropriate) they would undoubtedly enhance our ability to determine the realistic levels of confidence we could place in the non-invasive indicators. If a high level of confidence could be established for the non-invasive indicators, they could become an important part of the projected War Fighter Status Monitoring system.

THE SYSTEM SPECTRUM OF ISSUES AND POSSIBLE FUTURE RESEARCH SOCIO-CULTURAL FACTORS

It has been a truism in medical anthropology and medical sociology for the better part of this century that social and cultural systems play important roles in the perception of "the stressful" and in the organization of the response to that perception and to symptoms, illnesses and the longer term behaviors engendered by "the stressful". While risking oversimplification, a culture may be defined as the set of constructs, concepts, idea, and beliefs that provides its members, often in different ways, with their cognitive tool-kits for organizing, interpreting, measuring, and understanding the world and their environments.

One of the more critical roles played by culture is that of defining events as stressful or non-stressful. While there is wide agreement about hunger, thirst, heat, cold, and life threatening events as being life -threatening "stressors", there is far less agreement on the viability of many other life events, situations and stimuli as "stressors" or of being capable of evoking a stress response. In many cases the stressfulness of a stimulus is specifically attributable to cultural evaluations and expectations. A trivial but pertinent example might be the absence of flush toilets and privacy for bodily functions during a deployment. This might be stressful for Americans while it might be simply normal for citizens of a third -world country or for soldiers who are skilled and experienced deployers. Even more pertinent is the issue of crowding as a source for stress. In the deployment to the Persian Gulf, crowding was spoken of as a significant source of stress by a number of American service personnel. Many found the constant observation of their behavior by others, their inability to create personal space and to find privacy "extremely" stressful. [36] These responses are understandable in terms of American values and cultural expectations. Individuals brought up in other cultures, however, where densely shared living space and constant observation by others are norms, would not find "crowding" stressful. Unfortunately, some scientists all too often create templates based on their own cultural or sub-cultural biases and their responses to events such as 'crowding". Some investigators assume such "events" to be universally stressful for the population under study, rather that eliciting from the population, from the subjects themselves, their own perceptions of significant stressors and how the valences of those stressors are perceived. Many stressors are or are not deemed stressors because of the interpretation given them by the subject, rather than any inherent quality of the stressor. This can lead to the danger of misattribution or misperception of the sources of stress responses. We must remember that some members of the engaged population may even hold positive views of the terrors of combat.

Culture, Illness and Disease

A key component of culture for all humans involves the set of beliefs about functions and vulnerabilities of the body, about causes and potential threats of disease and about illness and bodily symptoms. These beliefs hold significance both predictively for the future and for the individual's status within his or her social group. As in all human populations cultural beliefs are not standardized. They do not come shaped by a "cookie cutter" but vary between individuals and amongst subsets of individuals. While in any given culture there is usually a "meta" set of shared beliefs and interpretations, there may be wide differences defining members of different subcultures. This can be particularly true of medical and illness beliefs which may be profoundly influenced by education, experience, religious belief, social status, ethnic background and so forth. It is also important to point out that widespread cultural beliefs, including ones that may play a significant role in the interpretation and perhaps the generation of physical symptoms need not be of great depth. Rumor, media assertions, newly fashionable beliefs may spread rapidly and play significant roles in the organization of behavior. These phenomena may lead to significant reordering of causal attribution and high anxiety about posited outcomes and risk among major segments of populations. They also appear to play a role in shaping the nature and interpretation of physical symptoms experienced by some individuals.

Historically, such cultural patterns and culturally mediated beliefs have contributed strongly to the organization and expressions of the behavior of soldiers in response to the stressors of deployment and combat. It has often been pointed out that the responses to stressful combat in World War I were most often organized around physical symptoms. The military culture of the period did not accord the same legitimacy to "psychological" symptoms that it did to physical ones as valid reasons for withdrawal from combat. Subcultural differences which underlay the bases of medical diagnoses in the British Army led to officers being diagnosed as "neurasthenic" and subjected to different treatment patterns than enlisted men who were diagnosed as suffering from "shell-shock" even though both shared many of the same symptoms. Similarly, in World War II, in the U.S. Army, transiently behavioral breakdowns were labeled as exhaustion and considered to be, at least in part, psychological in nature, but were a legitimate form of "short term" illness for most soldiers. In a number of elite forces, (Airborne, Ranger, etc.) however, soldiers manifested physical symptoms, rather than non-physical behavioral or mental ones. [37] Likewise the wide scale generation of gastro-intestinal disorders among troops in "Gastro-intestinal neurosis" by Weinstein and others.[38]

The illness responses seen in the above examples grew out of deeply held long-term beliefs and cultural patterns. However long held cultural beliefs are not the sole requisite for such responses. Thus in addition to the kinds of examples above there are a number of excellent examples of rumors and de novo-created beliefs which drove rapid development of symptoms or illness organizing behaviors in military populations. These include the response to atabrine/mepacrine in World War II where rapidly spreading rumors and suppositions about long term effects led to numbers of troops exposing themselves to malaria when they refused to take the drug.[39] Similar phenomena have been noted in civilian populations as well, and some of these both military and civilian will be discussed in a later section.

The cultural dimensions that appear to govern symptoms and responses to illness are neither simple nor one-sided but as illustrated above in the examples from World War I and II operate at a number of different levels. Perhaps the most widely understood cultural assumptions and beliefs about illness are those which tend to characterize groups by their members' symptomatic responses and their patterns of dysfunction. The examples from the British experience in World War I illustrate how a class-based medical culture responded to the same essential phenomena with two different diagnostic and treatment categories.

Studies of ethnic and income groups also reveal this pattern of similar illness phenomena given different diagnostic and treatment categories. In studies of different ethnic groups or markedly different S. E. S. (Socioeconomic Status) groups equally different patterns of symptom production and reponse have often been noted. In psychiatry, these class-based diagnoses, at times has led the medical community to assign different diagnoses for the same underlying ailment based on the group affiliation of the patient. (i.e., groups distinguished by a combination of income, education and job status.) In their classical study of patterns of symptom presentation in mental illness in the greater New Haven area carried out in the 1950s, Hollingshead and Redlich discovered that members of upper level S. E. S. groups presented their psychiatric problems primarily through psychological and behaviorally dysfunctional symptoms. Members of lower S. E. S. groups, on the other hand, presented what turned out to be the same sorts of psychiatric syndromes or diagnostic categories as sets of primarily physical symptoms and disabilities.[40] During the same period Zborowski's work on cultural differences in the perception and reaction to pain was also carried out. Zborowski studied members of four different ethnic groups at a Veterans Hospital in New York, persons of Italian, Jewish, Irish, and "Old American" descent. The study showed that the members of the different ethnic groups had different levels of expression of the experience of pain, and also interpreted its significance differently. They experienced different levels of anxiety and apprehension about their future health possibilities indicated by pain in the present.[41]

In the 60's Zola reported the results of differences in presenting symptoms on the part of people of Italian and Irish backgrounds admitted to certain clinics at Massachusetts General Hospital. While the disorders were the same, members of each group often attributed symptoms differently. One group tended to generalize symptoms and present their problems as general and diffuse while the other limited them to specific parts of the body. While both patterns could be seen in each group, each, as well, tended to have a dominant one. Since the ailments were the same for the members of each group the differences in perception and presentation demonstrate the power of culture to influence the experience and expression of symptoms.[42]

The role that cultural and social factors play in the development of symptoms of mental illness, as well, has been exceptionally illustrated by Dohrenwend, et. al. They have indicated that the perception of and response to stressful situations may play a contributory role.[43] Their research has been underlined by the work of Brown and his colleagues on the role life stresses appear to play in the genesis of depression with its protean array of physical and psychological symptoms.[44]

Cultural and social factors also play a substantial role in other forms of illness. Medical anthropologists have pointed out for some decades that while illness may be the response to an

exposure to an agent of disease or to a life-threatening event, illness is also a cultural construction of the response to an ailment or disease. The work of Kleinman and his collaborators has been of particular importance in elucidating the role of cultural variables play in the creation of the cultural construct of illness. In work done both in Asia and the United States, Kleinman focused attention on the "illness narrative". The illness narrative is the conventional way in which the response to a diagnosis or set of symptoms is organized, interpreted and presented to the world of medicine and to the individual's own social and cultural habitat. Such culturally defined behavioral narratives apparently serve to organize the patterns of perception, symptom presentation and anticipation(s) about the future. In a very real sense the narratives focus the attention of the individual upon symptoms that are deemed to be socially and culturally appropriate to the illness or its perceived etiology. The powerful contribution that such culturally defined narratives of illness can make has been well explicated by Kleinman.[45] . In our media-saturated society, it is also likely that "scenarios" for such narratives might be contributed to and constructed in part by the media. In this sense, the media may provide an "explanation" of cause and of presumed effects. The media even promotes generation of such symptoms in vulnerable individuals by underlining anticipated symptoms and focusing upon them.

On an individual basis it appears that symptom presentation to both the medical and social systems is shaped by both the beliefs about the assumed etiological agent and the manner in which the individual characterizes the pattern of symptoms. As Robbins and Kirmayer have pointed out: "Everyday thinking about illness appears to be influenced by underlying illness schemata or representations. Lay models of illness include ideas about identity, cause, time course, consequences and curability of condition. Schemata reflecting these ideas have been shown to explain patterns of self care, propensity to visit a doctor, delay in seeking help, compliance with therapeutic regimens, and success in coping with chronic illness."[46]

A prototypical example, which illustrates the way in which illness narratives, and cultural variables, function in epidemic mental illness the sickness known as Koro.[47] Koro was first seen in epidemic proportions among overseas Chinese in Southeast Asia and seen in mini-epidemics in other parts of Asia. Epidemics of mental illness do not require beliefs as dramatic as those which sufferers from Koro hold, that their penises are retracting into their bodies (despite visual evidence to the contrary). A number of sick building syndromes, for example, have produced prolonged distress and a wide variety of long-lasting physical symptoms in situations where no pathogen or toxin is detectable. In a number of these cases, what was detectable was an odd smell or the equivalent to which illness-creating power was attributed.

One of the most intriguing cases of cultural contributions to the creation of physical symptoms for many people over a fairly long period of distress was the nuclear accident at Three Mile Island[48]. It is obvious to any observer of American culture that nuclear energy and its associated radiation have come to represent a central threat to health and life in the consciousness of much of our population. While the Three Mile Island reactor accident and breakdown was referred to as a catastrophe at the time of the incident, and indeed years later is still referred to as a "tragedy" the physical and biological realities are much different. While the breakdown was catastrophic for the reactor, the event was not physically catastrophic for the community. The

containment systems worked and the release of radiation into the atmosphere was comparatively trivial. Nevertheless, there were significant long-term consequences for many in the surrounding community. Compared to control populations in other parts of Pennsylvania, the TMI population continued to report more prolonged physical symptoms and make more visits to physicians. The sense that "something bad" had happened was, of course, reinforced by the economic damage that many felt they had suffered due to the drop in property values secondary to the accident. Nevertheless, it is difficult to divide these fears from the beliefs in the general public about the insidious and threatening nature of radiation.

In a wider cultural sense, Kai Erikson's work on the aftermath of the devastating flood in the mining community of, Buffalo Creek pointed out the powerful distress-evoking consequences that take place in a community after a disaster[49]. In the wake of such events as Buffalo Creek or Aberfan in Wales[50], where a landslide devastated a mining community, it would appear that many basic cultural assumptions about the nature, predictability and benignancy of the natural order and of God break down and leave segments of the population adrift. The cultural assumptions that have served as organizing principles for defining the meaning of one's life are disrupted. The disruption appears to create conditions of long-term distress.

Research on how social and cultural factors contribute to the creation of symptoms and long-terms patterns of distress must keep two essential caveats in mind. The first is that there can be no "one-size-fits-all" approach to socio-culturally-defined stressors. The second issue, which will be dealt with in later paragraphs is that only a small percentage of people exposed to significant stressors will respond with a prolonged pattern of distress even when there is a consensus about how stressful the event or range of events was. It must also be borne in mind, as the example of Buffalo Creek demonstrates that the stressor itself may only be an initiating but not sufficient cause of the distress that follows. The long-term distress may be more clearly related to the changed perception of the world that the stressful event precipitated. Because of the variability of stressors and the differences in range of responses it is impossible to identify and target "universal stressors" in advance. From an epidemiological perspective, therefore, it is important to identify those events that are widely perceived to have a high probability of evoking a stress/distress response from an appreciable segment of a population. It is also important to document emerging events or changes in the evaluations of events in terms of how the individual perceives the potential threat. An example of how the evaluation of events changes is how segments of the American population currently view living near a nuclear generating plant. There has occurred a transition of belief from the 1950s when many Americans held a general belief in the benign and progressive nature of nuclear power. Many Americans currently believe that living anywhere near a nuclear power plant is to be living in a situation of prolonged and chronic apprehensiveness and "stress". This has been underlined in the last few months by the number of people in the South, living in the vicinity of nuclear facilities, reporting high levels of physical symptoms.[51]

The problem of current and emerging events that carry the potential for evoking distress is of particular importance for the military. In the period leading up to actual hostilities in the Persian Gulf War there was great concern and apprehension about the possibility of Iraqi use of chemical weapons. [52] This apprehension was strongly reinforced by a growing distrust of

American protective gear. This distrust and the beliefs, which accompanied it, shifted the potential use of the gear from a source of "protection" and stress mitigation to a source of stress and anxiety.

Thus the complex of issues around understanding socio-cultural stressors, including how they emerge, and how they change, underlines the importance of understanding the organization of beliefs about sources of potential psycho-physiological dysregulation held by the individuals in any military population under study. The etic grid, i.e., the set of beliefs about what is believed by physicians, researchers, staffs and pundits seldom has more than moderate correspondence with the emic grid, i.e., the actual belief structure of the population. It is also important to recognize that there may be significant cultural differences between those serving in different branches of the military. Soldiers' perceptions and beliefs can be driven by different combat scenarios and differences in how much technological protection they have. An example of the latter situation is apparent from interviewing soldiers during the Gulf War. Interviewers elicited (somewhat) different patterns of apprehension about possible combat events from "thin skinned" infantry units than from "thick skinned" highly protected armored units.[53]

We move on to our second caveat. Only a modest proportion of persons exposed to significant stressors will respond with prolonged patterns of distress, even when there is apparent agreement about the stress response-evoking qualities of the events. Thus we must move to an analytic concern for the sets of factors that distinguish these sub sets of people from each other and that may produce different outcomes. As Kirmayer's and Robbins' previously cited study on attribution showed, persons affected by stressful events will respond differently, some with somatic presentations and some with psychological presentations.[54] Researchers have gathered data on different factors which influence the quality and quantity of the stress/distress response. There is data on gender differences, personal hardiness, psychiatric status and psychological state, lack or presence of social supports, differences in arousal thresholds, autonomic lability, sense of mastery and control and a number of others.[55] It may be assumed that these factors interact with cultural and social constructs and beliefs. This interaction organizes what we might term, a perceptual and cognitive "doorway" and the secondary doorways beyond it that lead to different pathways and ultimately different outcomes. Perceived stimuli pass through and are guided by this doorway into one or another of the secondary doorways that open upon the biological pathways that generate the neurophysiological responses to the event or series of events in the brain and the body. The responses and their effects, electrical, hormonal etc., produce different classes of outcomes and symptoms. Obviously this is an area of great complexity, a complexity of a kind that makes rigorously controlled scientific research difficult, particularly in isolating the specific measurable responses and their relative weights in contributing to illness and/or somatic outcomes.

Certain cultural and social factors involved in establishing the cognitive doorway, can be isolated as a group, as a constellation, but their actual tenure and impact for any given individual represents a thorny problem. We have to confront this problem with the same depth and understanding that Brown approached the problem of stress-creating valences of life events.[56]

We know that social and cultural variables contribute to the probabilities of risk in populations but in terms of the probabilities of risk for any given individual, the cultural and

social variables must be viewed in terms of their contribution to shaping the cognition of and response of the individual to any given event or sequence of events. To conduct research in this area for an institution as large as the U.S. Army or its sister services, we should consider (minimally at least) a three-tiered approach in the design of our research. Such an approach would parallel the pattern of research and assessment used by the WRAIR Human Dimensions Research Team in Operation Desert Shield/Desert Storm but should both be more extensive and intensive than the WRAIR approach. Initially, sample ethnographic interviewing should be carried out in a number of military units, both combat arms and support, to ascertain the range of issues and probable events in both current life and future deployments that could be perceived as potential stressors. These should include both acute events and chronic sources of possible distress. This should be followed by a questionnaire study utilizing a representative sample to determine the distribution of such beliefs or concerns in the wider population as well as the kinds of valences given to them. In the event units are deployed the same kind of procedure (a combination of ethnographic interviewing and questionnaires) should be followed in order to catalogue perceived stressors in the deployment and then test these perceptions against the wider population of deployers and non-deployers. (With today's lightweight computer/scanner/word processing and printing equipment such studies involving rapid analysis and turnaround are highly feasible - as was demonstrated by the WRAIR teams in both Bosnia and Haiti.) These studies should be coordinated with those gathering both physical and psychological symptom data.

The second tier of studies should involve intensive interviewing of a small stratified sample of respondents in order to attempt to determine why the given events, situations and valences were chosen by them. The interviewing would attempt to disentangle idiolectic and idiosyncratic factors from more generic social and cultural ones as well as try to determine from the sample the differences between those who might be called "high stress anticipators" and those who are "low stress anticipators".

Following a deployment the third tier of the study should be put into action. This would involve looking again at a stratified sample, utilizing a matrix that might focus upon pre-low anticipation/low symptoms respondents who became elevated stress responders and presenters of symptoms, those who were high/high and mid-range who changed to high or low. The objectives would, be to determine how much of a role cognitive anticipation, actual events, or both played in the final outcome.

THE COGNITIVE DOORWAY

It is generally agreed upon that individuals' cognitive systems play an essential role in determining the response to many stressors. The next segment of the system that we might focus on could be called the shape and processes of the cognitive doorway and their consequences for internal bodily events.

There are many questions that arise in trying to formulate the shape and processes of the cognitive doorway. Cognitive processes are to a significant degree shaped by both the structure of language and the semantic and personal historical meaning (the ideolectical meanings) assigned to words, metaphors and other descriptors. An initial issue, for which there is very little

data, is whether or not the brain processes words or concepts that are affect- loaded for the individual differently from other stimuli perceived as carrying lesser or neutral valences. The question is whether or not such differences in the emotional valence of a stimulus leads to the activation of different neuro-endocrine/neurophysiological outcomes. The possible consequences of the activation of gates by verbal stimuli that then open to different pathways appears to be moot. It certainly requires further exploration.

Work carried out by Chapman, et. al., using Osgood's Semantic Differential Scale, indicated that there are neural differences in response to verbal stimuli, opening a fruitful field for further research. Chapman and his colleagues used stimuli words from Osgood's Semantic Differential Scale and demonstrated that it was possible to determine the categories of the "connotative class of word" involved. Words were denoted as belonging to Osgood's categories of "evaluation", "potency", or "activity" from Brain Evoked Potential responses. Chapman and his colleagues' work also demonstrated clear Brain Evoked Potential differences between "content" and "function" words.[57] If such differences were discernible with the modestly emotionally or evaluatively laden adjectives of the Osgood Scale, it would certainly suggest that descriptors of "stressor event" stimuli might also elicit detectable patterns of response as well as measurable differences between levels and patterns of arousal. While Chapman noted that there were readily detectable individual differences in "Brain Evoked Potential" responses, he and his colleagues pooled their data because their interest was in categories of response and not in individual differences. It would be of potential diagnostic interest to determine whether such individual differences indicate differences in longer term patterns of response which then lead to possible longer term psycho-physiological distress and symptom generation and maintenance. It would be of equal potential clinical interest, utilizing more recently developed technologies such as PET scans, to determine whether or not such individual differences, if they exist in significant degree or kind, indicate differences in either brain pathways or in the intensity or extensiveness of response. We could envisage a multitude of laboratory, small-scale prospective studies and retrospective case control studies that could illuminate this question.

Measuring responses to verbal stimuli, however, would only reveal a partial picture. We must remember the powerful interactive cognitive/physiological complexity of the entire area. This is particularly illustrated in a study by Bonanno, Keltner et. al. The scientists studied a population for patterns of emotional avoidance. Emotional avoidance was measured by the dissociation between verbal and autonomic responses to grief over time which was in term measured by verbal responses to grief stimuli and autonomic arousal. They demonstrated that "verbal avoiders", while exhibiting autonomic arousal in the test situation were at lower risk for longer-term somatic and other symptoms than were "non-avoiders" whose emotional responses were not dissociated from autonomic arousal.[58] This set of relationships between physiological variables such as autonomic arousal and the verbal emotional variables should be investigated. In tandem with such investigations of neuro-physiological dimensions of the "cognitive doorways" created by various stimuli, both historical and dimensional analyses of the stimulus events could be profitably undertaken. Osgood's Semantic Scale or similar instruments for "mapping" the connotative dimensions of stimulus words or event descriptors could prove valuable in determining individual differences both of levels of arousal and duration of

dysregulatory activity.

It is possible that such studies, if combined with in-depth interviewing which elicits life history material and other psychological and physiological measures may enable us to factor out the differences between those persons whose symptoms lead them to enter medical care systems as opposed to those who view the same symptoms as part of the "wear and tear" of life. These studies may also give us further clues to the processes of amplification, explored by Barsky, et. al. and amplification's role in creating further dysregulation and possible dysfunction[59]. Barsky's work has demonstrated how psychosocial and personal factors can lead to the perceived intensification of symptoms as well as increase in the number of symptoms.

Barsky's work, as well as that of others, also leads us to re-examine the "cognitive doorway". We might metaphorically subsume amplification as the possible deepening and widening of the "cognitive doorway" leading to intensification of initial physiological dysregulation, intensification of symptoms and of symptom production by *post hoc* causal attributions. These causal attributions indicate more threatening outcomes than the initial symptoms themselves might have done. Beliefs about the "meaning" of symptoms, their causes and the possible negative outcomes for the individual may sustain and broaden a pattern of illness for which there is no really threatening physical cause. The implicit threat of attributing symptoms such as joint aches and pains to osteoarthritis or aging is, we would posit, far less likely to lead to and sustain prolonged autonomic arousal than attribution to toxins or pathogens.

It is critical, in this context, as in others, that we constantly keep in mind, that we are dealing with a system made up of multiple ongoing processes rather than a simple correlative assertion of a relationship between two singular events, such as "exposure to traumatic event at time "A" equals symptom production at time "B". It must be remembered that human beings are characterized by physiological and mental processes that take place continually in time and duration. These processes are open systems subject to both feedback and feed forward information as well as "new" information from the outside that can alter processes, patterns and activities. Symptoms that may have no grave or threatening clinical value may parallel others that do. One set of constructs may replace another in the subject's meaning system. In psychiatry, it is a truism, for example, that physical symptoms attendant upon anxiety and depression are protean. They can "mimic" a wide range of threatening illnesses and diseases. An example that has troubled military medicine for some decades has been the fact that symptoms of the fairly high level of anxiety that are common in troops entering combat parallel those of initial exposure to nerve agents. A question raised by many, was whether or not troops would disable themselves with injections of atropine and become dysfunctional believing that they had been exposed to nerve agents, when it fact they had not.

This issue of the psychological generation of physical symptoms brings us to four interrelated cognitive areas that appear to require far more rigorous study than has been devoted to them. They are:

1. Belief
2. Suggestibility
3. Hysteria
4. Psychological contagion.

THE PROBLEM OF BELIEF

The problem of belief, that belief functions both as a process and as an end state that has extensive consequences, is one of the most poorly investigated subjects in the understanding of human behavior. The problem of belief is critical, we think, in understanding processes of attribution. It is also an essential factor contributing to suggestibility, hysterical symptom production and contagion. An illustration of the importance of understanding the role of belief and its influence on related cognitive areas comes from the work of the 19th century American philosopher, Charles S. Peirce.

In dealing with the concept of belief in the "real" world we think that the basis for definition and consideration should start with Peirce's writings. Peirce stated: "I hold that what is properly and usually called belief, that is, the adoption of a proposition as a possession for all time . . . has no place in science at all. We *believe* the proposition we are ready to act upon. *Full belief* is willingness to act upon the proposition in vital crises, *opinion* is willingness to act upon it in relatively insignificant affairs."[60] In an earlier essay (1877) Peirce pointed out that "That which determines us, from given premises, to draw one inference rather than another is some habit of mind, whether it be constitutional or acquired. The habit is good or otherwise, according as it produces true conclusions from true premises or not; an inference is regarded as valid or not, without reference to the truth or falsity of its conclusion especially, but according as the habit which determines it is such as to produce true conclusions in general or not."[61]

Peirce's reasoning on beliefs has been borne out in this century in a great amount of work by anthropologists, psychologists, linguists, etc. Their work has demonstrated that human essentially think, reason, and attribute causality in the same way, the primary differences being the assumptions or beliefs which serve as the premise for reasoning. The process that dictates that B is or was caused by A may or may not be governed by empirical evidence. If one's belief is that germs, viruses or toxins cause an illness than the "A" invoked as the causative agent is clearly very different that would be if the belief was that the Deity, witchcraft or "stress" was the causative agent. Peirce's major point in many of his essays was that science is founded upon doubt rather than belief. Significant problems exist when beliefs preclude the possibility of change in response to empirical evidence. While we are all aware of these processes that stem from belief we have little substantive knowledge of what might be termed the psycho-neuro-biology of belief. This is particularly true of the processes by which a belief is fixed and becomes the basis of both attribution and interpretation of events and "crises" in the world of the individual. We have little sense of how such processes appear to "lock in" and create bounded or even single] channels for the interpretation of both sensory and cognitive data.

It is this "locked in" aspect of belief that is pertinent to our concerns. The beliefs are "locked in" when they preclude the processing of any data that might question or undermine the "causal" paradigm governing an individual's choices or behavior. Some gross examples might be individuals in the United States who continue to believe that the earth is flat or the considerable number of people who believe that the 1969 moon mission and walk was "faked" in Hollywood. More significant to our research concerns would be the consistent denial of the extensive epidemiological data demonstrating no relationships between silicone breast implants and, autoimmune diseases, by persons both claiming and demonstrating physical symptoms.

Other examples include claims of exposure to Agent Orange and the development of subsequent illness and symptoms, or the "dangers" of living in the proximity of high tension electrical lines and a number of other like phenomena. In several of these cases, the intensity of peoples' beliefs in the causal relationship between the exposure and the development of symptoms has been reflected in societal and political decisions to define a causal relationship where none can be scientifically demonstrated. Obviously, such confirming behavior from loci of authority will serve to reinforce and harden belief and further exclude the possibility of acceptance of contrary evidence or even the admission of doubt. Once again, we must wonder, as to whether or not there are significant neurobiological aspects to such fixation of belief that lead to either not attending to or not processing of alternative information. Some initial experimental attempts to look at this problem were begun in the early 1980s by the late Dr. Donald McKie of Keele University in the U.K. focusing on brain electrical activity, but they were cut short by his death[62]. Present-day theories about the role of inputted information in the structuring and restructuring of brain wiring along with cultural and psychological research, might allow for other kinds of research using both the B.E.P. (brain evoked potential) and more recent technology like PET scans. These tools can look at whether strongly held antecedent beliefs affect the way the brain handles both information consonant with a set of beliefs and information that challenges the antecedent beliefs.

SUGGESTIBILITY

Suggestibility might be defined operationally as the human propensity to "believe" in certain things or relationships without recourse to empirical validation of the "belief" in question. Asch's work on conformity demonstrated that in the college classroom student groups could be manipulated into agreeing to phenomena that were patently not so.[63] While there have been challenges to Asch's work, the research demonstrated that social pressures could be generated in small group settings that would lead the majority of the group members to agree with that which sense evidence said was not so. The vulnerabilities, structure and dynamics through which an assertion by "A" will be turned into a fixed belief by "B" has not been subjected to rigorous psychosocial or neurobiological assessment and analysis in recent years. The roots for such work might go back to the pioneering studies of propaganda and its impact by Lasswell, Lazarsfeld and others during World War II.[64] Rigorous study, particularly in real social environments has, however, been sparse, at best because individual variation in levels of suggestibility is obviously wide. Similarly, there are a large number of sources whose assertions will be taken as matters of completed fact and internalized as "beliefs". The large number of sources and the rates of individual variation make rigorous testing difficult. The allocation and acceptance of such sources has also not been a matter of extensive study. The credibility granted to sources like parts of the media or friends and authority figures has yet to be examined in regard to how this credibility affects both individual and group perceptions of fact or truth. We need to study more carefully who and what is to be believed, what is prima facie evidence, and what items of belief block acceptance of contradictory or competing data. Both suggestibility and belief may, therefore play significant roles in determining the valence and impact of an event as stressful.

If suggestibility is an important parameter in determining the potential "stressor" value of

external events and stimuli, then efficient and cost-effective ways of assessing and measuring suggestibility need to be developed. In order to develop accurate measuring scales we need to investigate how group structure and group pressure towards conformity creates belief and conduces to action. Investigations into group structure, group pressure, conformity and belief leading to action follows the work of Asch and Milgram.[65] The question that might be raised is how much of the suggestibility exhibited by humans is a product of attempts to maintain a comfortable and legitimated position in a group. This is of particular importance in military groups given the (well-established, since World War II) role of primary and small groups to the individual.

Another aspect of suggestibility is the phenomenon of "recovered memory" where (in the overwhelming number of cases) the memories, recovered or repressed were in fact false memories. These "memories" appear to have been implanted in therapeutic, quasi-therapeutic and other social settings. In a manner loosely analogous to the Asch and Milgram studies, we might postulate that these phenomena of "recovered memory" serve as modalities in which the brain reorganizes and classifies notional and false data. The brain classifies these data as existential and real in response to both external and social demands perceived as emanating from authority figures and/ or professionals with expertise, as well as from the personal predispositions of the subjects.

A second issue that might be investigated in looking at suggestibility is the question of brain function in the assimilation of false data perceived as real phenomena. Here if we take a perspective drawn from the work of Chomsky and Levi-Strauss as well as early experiments in operant psychology we might consider the following and look for ways to approach this problem. That is, will brain wiring and inherent modes of taxonomizing (naming and cataloging words and events) lead to defining relationships that may not be so in the empirical world? In a sense the question takes place in a universe that from a scientific point of view is often defined by adventitious and coincidental happenings. Does brain organization and function lead to making causal and correlational relationships where these do not or might not exist? Is the functional thrust of the brain for ordering and patterning thought like that of its functional thrust for language - to create structures that make sense even if they are empirically invalid?

The brain's ability and tendency for this functional ordering may be particularly true if there are external sources or people who appear to validate the process even if the validation occurs infrequently. A view of this process may be gleaned from early human work in operant psychology. The investigator presented random numbers to subjects who were supposed to respond to each with the "right" number in return. There was no correct response. The investigator, however, discovered that even the subtlest form or reinforcer, if emitted about 15% of the time, instilled the notion in the subject that a set of correct responses existed, that the pattern of these correct responses had been discerned and the responses henceforth given were all correct. This leads to the question. What is the relationship between the valence (status, potency, power of assertion, relationship) of a source, to the number of reinforcers for that source, that lead to the "fixation" of a belief, such as the belief that there is a causal relationship between phenomena (such as low exposure to Agent Orange in Vietnam and present illness), particularly for certain individuals and subgroups. A schema [that looked at valences of sources

and the number of reinforcers of the sources might help us understand differing levels of suggestibility.

A final method of assessing levels of suggestibility might be the use of hypnosis. Several investigators view hypnotizibility to be an entirely a function of the level of suggestibility of the subject. Obviously for reasons of expense the determination of hypnotizibility would not be an effective measure for epidemiological studies. It could, however, serve as a tool for building contrasting paradigms based upon psychological and physiological attributes that characterize different classes of subjects from the easily hypnotized to non-hypnotizable. The goal of investigation into these areas should be the creation of a cost effective and simplified suggestibility assessment instrument. The investigation can be carried out using moderate numbers of subjects and intensive techniques of investigation. Where appropriate, investigators should use assessments of brain function with technologies such as BEP and imaging as well as more sophisticated neurophysiological measures. The instrument created for assessing suggestibility should, if hypnotizability does distinguish differences in suggestibility, have both validity and reliability for use in epidemiological assessment and as a clinical aid.

HYSTERIA

Hysteria no longer appears in the DSM's having been reallocated to either conversion or dissociative disorders. This is due in good part to the highly pejorative meaning the term acquired through the centuries. To many it often represented a combination of mental and physical weaknesses combined with irrational and hyper- emotive behavior. As a system and as overt behavior however, rather than as a distinct diagnostic category, it represents an area that should continue to command our attention.

If we go back to the reformulation of hysteria by Babinski and by Hurst and others during World War I, we might define hysteria as the process or system of transformation in which the experience of the event either directly or referentially is transformed into symptoms, either physical, mental or both in response to the expectations created in interactions with external authorities. Babinski saw the primary transaction as one of collusion between the physician and the patient in which the patient produced classic hysteriform symptoms in collusion with the physician's expectations as to what those symptoms should be[66]. In today's world the transactional area between patient and authority figures has broadened widely. Presumably authoritative information now comes from a vast number of supposedly knowledgeable sources. This process or transactional exchange central to "hysteria" impresses us as truly critical since it embodies both belief and suggestibility. It has been described both clinically (for centuries) and literally, but like many other phenomena under discussion here, it has been poorly understood as a psychobiological or neuro-biological process. To Hurst treating combat stress cases (then denominated "shell-shock" or war neurosis) in the British Army in World War I hysteria and its embedded transactions was seen as central to the creation of casualties[67]. What is less understood is the way in which referential stimuli, the stimuli revealed by the doctor or some other authoritative source, thus prompting the transaction between the authority and the patient, central to the concept of hysteria, may also aid in the generation of combat stress or PTSD symptoms following the combat event.

Much research has been done on PTSD. As a result of the work done on responses to acute combat stressors there is some understanding of possible consequence of acute traumatic exposure to combat but the mechanisms involved in symptomatic responses to referential stimuli, however, are basically known only anecdotally. These mechanisms might be described in the following modality. "X years ago at work, or during a deployment, etc., you were exposed to an illness causing agent. You demonstrated no awareness of it and it produced no symptoms at the time. However, we believe that this exposure is responsible for the following symptoms, some of which you have undoubtedly experienced." It is quite possible that there was no such exposure or at best a sub-clinical exposure incapable of producing either the proximate or longer term symptoms after a long hiatus. Yet it is a common experience that a number of individuals anywhere near that place at that time will start presenting defined and real symptoms or complexes of symptoms. They will be attributed to the exposure, which may not have taken place. The symptoms may even become disabling for some. As in the classic descriptions of hysteria following Babinski[68] the question that faces us is what happens psychophysiologically when the individual is told by "authorities" that he or she is, or should be, experiencing certain symptomatic reactions because of an exposure which may (or may not) have taken place.

These issues of hysteria, suggestibility and belief are extremely difficult to investigate scientifically. There are wide numbers of intervening variables and difficulties that would be encountered in setting up any prospective studies. Retrospective case control studies might be a reasonable way of attempting to come to grips with some of the factors involves. Animal models might be created and be of help in attempting to define neurobiological differences between animals which shape to respond to a referential symbol evoking "stress responses" and those animals in the series which do not shape and so do not respond. The differences in neurophysiological response would provide clues to human experience.

CONTAGION

The dynamics and processes that underlie the phenomenon of psychological contagion are not well understood. On one level, we can assume that all of the processes we have just outlined play important roles. There are a vast number of examples of psychological contagion attended by symptomatic responses ranging from epidemics of witchcraft from the Middle Ages until recent times to some of the widespread cases attributed to "Sick Building Syndrome" or exposure to other, often non-existent pathogens. Examples range from the classic "Gasser or Phantom Anesthetist of Matoon" to any number of present school or business-based epidemics. Such phenomena have also been seen in small-scale epidemics in deployed troops a number of times in this century. In the past such epidemics were normally labeled instances of "mass hysteria" - a categorization but certainly not an explanation.

In the Matoon, Illinois example, following the fainting and hospitalization of one high school student, an epidemic took place in which a number of students were hospitalized with similar symptoms. In most cases the epidemic was precipitated in the student's home at breakfast time. It was believed, by the victims, that a "mad gasser" or "phantom anesthetist" had sprayed a toxic gas under the door that led to the victims's collapse. Sounds pertinent to the event were recalled but no trace of either the 'gasser' or of a toxic agent was ever found.[69] Some

cases of "sick building syndrome" have involved actual toxic environments in which symptoms-causing agents were circulated or recirculated through climate-controlled buildings and some have even involved legionella, but in many cases no trace of a toxin or pathogen was ever found. These cases did, however, often involve the presence of "chemical smells" usually from adhesives or other materials used in decorating or redecorating the building. These substances were found to have no possible toxic effects yet they "produced" persistent physical symptoms in numbers of people - often classed as the result of 'multiple chemical sensitivity" or 'chronic fatigue syndrome'. [70]

Three Mile Island might be another example in which a significant segment of a population produced a range of fairly persistent symptoms despite the fact that the level of radiation released could not produce illness. Reviewing the literature, with particular attention to the work of Dohrenwend and Baum cited above, and Erikson's work on Three Mile Island, it is apparent that the processes and dynamic of such epidemics are extremely complex despite the fact that their eruptions appear to be comparatively simple.[71] These examples portray situations in which an assertion or a "behavioral exemplar" or model appears to be converted through a combination of anxiety, fearfulness and suggestibility into the manifestation of symptoms or increased risk for illness or diseases. A pertinent military example for World War II deals with the refusal of soldiers in the Pacific and Burma Theaters to take atabrine in response to rumors about its terrible long-term effects.[72] The individual, interpersonal and social communicatory mechanisms involved in the World War II example and in more contemporary ones are not well understood. Contextual variables are also not well understood. These variables which lead to fixation of belief and the rejection of data demonstrating that pathogenic exposure did not occur or occurred or at too low a level to produce the consequences felt and exhibited in examples like Three Mile Island, may then spread rapidly throughout the group or community.

Social contagion is extremely difficult to study prospectively because we normally become aware of it at a point at which the process has either spread extensively or is complete. It is perhaps feasible that an experimental paradigm using psycho-physiological assessment of individuals, a contemporary version of Bales' interaction analysis, and a core task involving the development of belief in something that "is not so" (perhaps a variation of Asch's work) might be developed for close analysis.[73] A complimentary method might involve a deep, thickly descriptive post-hoc ethnographic analysis of such a situation, building in part upon a temporal sociometric communicatory analysis, e.g. "Who said what to whom, when, and with what result" combined if possible with psycho-physiological assessment. These are just two examples.

We are sure there may well be a number of other approaches, some of which may be far more fruitful than those suggested above. We believe that behavioral contagion is an important issue in terms of understanding the phenomena that are of concern to us. It remains a difficult subject, one that has been more honored by anecdote than by multi-dimensional research.

PSYCHOSOCIAL STRESSORS IN RELATION TO "DECREMENTALS'

For the sake of simplicity in conceptual organization we will use the term "decrementals" to describe those classical "stressors" that in and of themselves, particularly in physically healthy individuals, do not include psycho-social stressors that are perceived as possible threats to life or

limb, or threats to status, "self-esteem" or "self-worth" because of task failure. Nor do they encompass perception of involvement in events of significant traumatic valence. Decrementals might include such phenomena as sleep- deprivation, intense physical load, prolonged, continuing moderate to heavy physical load, continual over- exercising (which recent reports indicate may cause deleterious physiological and immunological effects), and like phenomena. One question to be asked is whether or not such "decrementals" in and of themselves might generate significant long-term physiological effects that would ultimately express themselves in some segment of the population as psycho-physiological and somatic symptoms. Should such "decrementals" be studied as singular, able to control for both situations and subjects where the perceived levels of psychosocial stressors are simply those of "background noise" of daily living? There is for example, evidence that prolonged sleep-deprivation has deleterious effects on immune function.[74] To the best of our knowledge, however, no data indicates that these effects are prolonged and lead to longer-term distress. Perhaps the more important question is whether or not chronic psychosocial stressors interact with single or multiple "decrementals" in such a way as to increase the probability of long-term distress that then has multiple symptomatic sequelae. We might wonder whether the combination of chronic moderate sleep-deprivation and high, continual and sustained workload combined with the multiple psychosocial stressors perceived in Operation Desert Shield/Storm helped create the psycho-physiological circumstances conducing towards long-term somatic and other symptoms in some veterans.

Another and more highly speculative possibility is whether or not such chronically stressful conditions combine in their effects on the immune system to create windows of opportunity for pathogens that otherwise might not be able to attack the individual effectively. There has been some work with humans that shows that stress creates a greater risk for infection with cold viruses.[75] Paul Black's provocative work with mice in a high stress model producing sequelae that strongly resemble autoimmune joint pathology represents a possible animal model approach to studying such phenomena[76]. Unfortunately, the range of viruses that appear to be capable of re-engineering the immune response to include significant longer-term autoimmune symptoms is not yet well defined or described. Another possibility to consider about how stressful conditions can affect the immune system is Ewald's theorizing about infectious agents which are latent and pre-existing but presently quiescent. These agents respond opportunistically to the vulnerability created by decrementals to become actively injurious.[77]

STRESS AND HEALING

The work of both Ronald and Janet Kiecolt Glaser on the relationship of stress to wound healing and to the establishment of antigen levels is both extremely provocative and of potential operational significance.[78] It would be important to build an experimental program based upon their research. Using their work, we should sort out and control for all the following factors, except that of proximate exposure to acute traumatic stress: the contributions of physiologically decremental activities, primary psycho-social stressors such as anticipatory anxiety and fear, severe conflict with a spouse or lover, etc., and secondary psycho-social stressors (events that may be happening at a distance such as concern over family matters). Such phenomena as these stressors and their impact on antigen levels, might contribute to our understanding of the process

by which environmental factors interact with human individual variation to contribute to the non-universality of immunity conferred by certain vaccines. While "herd immunity" protects the population and prevents significant epidemics from occurring in a protected population it does not necessarily protect the individuals soldier - it simply lessens the probabilities. The networks of potential contagion and infection that the soldier in combat may be exposed to, consists not only of his own population of protected colleagues, but of possibly unprotected civilians. The effects of stress on antigen levels should be of interest for two reasons, first because of the amount of time and levels of effectiveness lost to illness and disease in many combat situations and secondly, we might speculate if such low antigen levels contribute to sub-clinical infections they might have untoward long-term somatic sequelae.

The issue of wound healing and the length of time taken to accomplish it is obviously one of interest to the armed forces. It is of special importance for humanitarian reasons. Militarily the study of wound healing offers the possibility of shortening the impact of combat on what may be scarce medical resources, and it would allow for the return of personnel to their tasks more quickly than in the past. Better wound healing treatment may be of particular importance in the combat theater since the extensive use of body armor has shifted patterns of survivable wounding from the torso to the limbs. When we treat individual soldiers in the combat theater, we have to remember that it is difficult if not impossible to withdraw individuals from the psychosocial as well as environmental stressors of the combat theater without creating potentially adverse individual, unit and operational effects. The potentially adverse effects dictate a need for proximate interventions. Two possible lines of research may be considered here. The first might involve the development of research within the framework of liaison psychiatry for rapid "detoxification" of the psychologically traumatic effects of the wound itself. The second would involve the support of more extensive research into the fundamental biological relationships of "stress response" and wound healing. This would be directed at the discovery of the biological mechanisms involved in wound healing that may be subject to alteration through the psycho-physiological response to stressors. If biological or biochemical agents that could be found that speed wound healing by even modestly, it would represent a major benefit to the solder, the armed forces, and to society in general.

We might consider that research in the wound healing arena should rest initially on three kinds of studies: 1) animal studies structured to have some relevance as models to military situations, 2) controlled human studies, 3) studies of the "natural history" of healing and recovery.

An animal model that might prove fruitful if extended would be the "defeated hamster model" developed by Meyerhoff and his collaborators. [79] This model could utilize standardized wounds inflicted upon the "defeated hamster" (the chronically stressed animal) and various controls. Others animal models which might be considered involve variation of the learned helplessness models and appropriate controls, or ejected formerly "alpha" animals and controls in monkey colonies. These animal studies can be organized to enable the investigators to check for such decremental effects as changes in sleep pattern, food intake, etc. and their possible effects on healing and immune system alterations. They would enable researchers to explore in depth basic neurophysiological processes and alternations of supposed "normative" systems

invólved in wound healing. This would allow the possibility of developing interventions. This work might also help clarify and establish if there are equivalent processes in humans affecting wound healing and whether there are valid indicators of these processes, which can then be inferred from either non-invasive imaging or from minimally damaging procedures, since the physically destructive analyses from animal models cannot be carried out on humans.

The controlled human experimentation would obviously be most fruitful if carried out in the mode and the methodology established by the Glasers.[80] The bulk of the work would have to be laboratory-based but not all of it. We might consider extending this kind of work, (e.g., the Glaser's stress/wound healing research or stress/vaccination antigen model) to troops before, during and after Ranger Training, deployment to the NTC or JRTC or an intensive and potentially threatening overseas deployment. Again such possible decremental contributors such as sleep deprivation, climate, dietary alteration and other variables could be to some degree controlled for through the use of actigraphs, heart-rate monitoring, and health and food substance ingestion diaries, daily behavioral check lists and other instruments. Such investigations might also, ultimately be able to test for the presence or absence of additional non-injurious indicators developed in the animal studies.

Human natural history studies present another arena of research that could readily be carried out. While traumatic injuries could seldom be controlled and equal, as in the Glasers' studies, they could be judged in terms of "clinical equivalence" e.g., the number of stitches required, simple or compound fracture of the ulna or tibia and so on. Again the process could involve a combination of monitoring of the wound healing, and close monitoring of psychological, behavioral, physiological and "decremental" aspects of the healing period.

CONCLUSIONS

In this report we have attempt to lay out some of the critical issues as well as possible directions for future research programs that should be seriously considered by USAMRMC. As we have pointed out, at the present moment we face two essential problems. First, the present state of psychobiological knowledge is such that explicit, fine grained, research on the relationship between stressful exposures and long term somatic symptomatic outcomes would not be productive. Too much of the basic knowledge required about the systematic relationships between psychosocial phenomena, particularly in regard to what we call "stress", and their neurophysiological impacts remains in the category of "not yet known". Equally, a review of the literature demonstrates that, while in the course of the twentieth century, military psychiatry has had a degree of positive impact and success in mediating the effects of the stresses of combat and deployment these successes have primarily been the result of trial and error. They have seldom been the fruit of dynamic and systematic understanding of the behavioral and biological processes that were actually ongoing. The real successes of combat psychiatry in the course of World War II, the Korean Conflict and the Vietnam War were intellectually and "scientifically" grounded on assumptions and suppositions that were and remain unsubstantiated rather than empirical facts. Indeed, levels of success did not vary dramatically when, in different theaters and treatment centers equivalent outcomes were produced by marked different modes of therapeutic

intervention. The "real" nature of success becomes somewhat questionable, particularly in W. W. II and Korea, when we note that a majority of those returned to duty following a diagnosis of "combat fatigue" were returned to limited support functions not combat roles. The real achievements are a testament to improvisation, practitioner commitment and empathy, human resiliency and, perhaps, the placebo effect. They are, unfortunately, not a testament to profound scientific understanding. This is not a critique of those who pioneered modern military psychiatry; for the scientific bases for intervention and treatment were not defined in many cases other than as hunches and assumptions. In the era of mass armies, high personnel redundancy and of widespread national cultural expectations that war could take a permanent psychological as well as physical toll, limited parameters of success were acceptable in a way that no longer exists. A small, highly trained, high technology, minimally redundant, force remains exposed to the same stresses that afflicted the force that preceded it, but also to some new forms of stress. Since its personnel are not rapidly or easily replaceable we require modes of prevention and treatment that need far higher probabilities of success than those of the past did. Since such a force must be as resilient and as sustainable under severe stress as possible we would contend that we must aim at the kind of knowledge that will lead to success levels equivalent to those of good vaccines or antibiotics. The lessons of Gulf War Illnesses and Post Traumatic Stress Disorder following the Vietnam war as well as those of combat stress reactions in previous wars should direct us to the necessity of developing the kinds of integrated research programs that we have indicated are required. This need is further underlined by the, now widespread national sense that it is our responsibility to ensure that when men and women are returned from combat and stressful deployments we will have effectively intervened to prevent the development of long term psychophysiological symptoms and consequent disabilities. We believe that we have indicated possible sets of pathways that would aid in moving towards this goal. In some senses it represents a discontinuity and movement away from practices of the past but we believe that it is a necessary one.

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